Confirmations and truthlikeness. Reply to Gerhard Schurz

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CONFIRMATION AND TRUTHLIKENESS

REPLY TO GERHARD SCHURZ

The subtitle of ICR reads: "On some relations between confirmation, empirical progress, and truth approximation." Gerhard Schurz's contribution is in fact a critical review of my account of (hypothetico-) deductive (HD) confirmation and of truthlikeness. The former account was intended to be a partial explication of confirmation and the latter is a prerequisite for discussing the prospects of truth approximation by standard procedures, notably the HD method. Assuming revisions of the definitions of HD confirmation and truthlikeness, Schurz suggests he wants to connect them in a similar way as I do by conceiving the "rule of success," leading to empirical progress, as the glue between confirmation and truth approximation. Two of the major claims of ICR are that the rule of success is typically of an instrumentalist nature, for it takes counterexamples into account in a non-falsificationist way, and that this rule is straightforwardly functional for truth approximation. I am very happy with Schurz's support of this general line of argumentation in favor of HD evaluation instead of HD testing. However, I also have to concede that his criticisms of explications of HD confirmation and (basic and refined) truthlikeness are rather severe, so I hasten to respond to them.

As a matter of fact, Schurz's criticism of my account of HD confirmation is of a different nature than that of truthlikeness, although the same notion, viz. "relevant-element," is used for both purposes. My explication of HD confirmation is criticized for being too weak, whereas that of truthlikeness is stated to be on the wrong track. I shall deal with the two notions separately.

Confirmation

The arguments Schurz states to deal with my account of HD confirmation are the following: it leaves room for weird examples, viz. (7)-(9); it doesn't satisfy the "consequence condition" (C-H); and it is not (necessarily) ampliative.

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Schurz's remedy amounts to a sophisticated strengthening of, let me call it, naïve deductive confirmation, viz., hypothesis *H* logically entails evidence *E*, by requiring a number of special logical conditions on the relation between *H* and *E* in terms of "relevant elements." He also explains that the strengthening of naïve deductive confirmation in terms of "(the content parts of) a natural axiomatization," proposed by Ken Gemes, is weaker than his own. However, the question of course is whether we need any strengthening at all, so we shall have to consider the three arguments against the "liberal account" put forward by Schurz. I shall briefly comment on these arguments and add an extra argument in favor of the liberal account.

It is indeed very easy to produce weird examples, like (7)-(9), but it should be realized that, historically, similarly weird examples can be given that we have come to appreciate. For example, consider all deductive confirmation of Newton's law of gravitation, assuming the three laws of motion, by (conditionally) deductively well-confirmed empirical laws dealing with freely falling objects, trajectories of projectiles and planetary orbits. This was (naïve) deductive confirmation of a hypothesis that was originally conceived by leading scholars as very occult, notably by Leibniz and Huygens (see Atkinson's contribution to the companian volume, for a concise statement). According to Schurz we should say instead that all this evidence did (almost) not confirm the hypothetical law of gravitation according to Leibniz and Huygens, whereas it did in the eyes of Newton. In sum, the price Schurz wants to pay is that (deductive) confirmation does not apply to hypotheses that are considered as weird and, hence, it is a subjective matter.

Schurz's second argument against naïve deductive confirmation is that it does not satisfy the consequence condition: if *E* deductively confirms *H*, it does not automatically deductively confirm any consequence *H′* of *H*. Schurz also reports my localization argument relativizing the, probably, counterintuitive fact that naïve deductive confirmation has the opposite "converse consequence condition": if *E* d-confirms *H* it d-confirms any strengthening *H″* of *H*. Indeed, if both properties were to hold, any *E* would confirm any *H*. However, the *prima facie* desirability of the consequence condition can be explained in another way. Let us consider the previous example of confirmation of the theory of gravitation by the (corrected) law of free fall (LFF). Naïve deductive confirmation leads to the following claims: assuming Newton's theory of motion (TM), LFF not only d-confirms the law of gravitation (LG) but also that law in conjunction with a hypothesis that even now is still considered to be weird (WH). Moreover, assuming LG, LFF d-confirms TM, whereas, even assuming TM, LFF does not d-confirm WH (localization). In other words, when *E* d-confirms *H*, irrelevant subhypotheses can be localized and relevant subhypotheses are (at least) conditionally

d-confirmed (in the sense indicated). To be sure, in a detailed analysis of a particular case the natural axiomatization of Ken Gemes can play an illuminating role.

Let us finally turn to the quest for an ampliative account. Schurz rightly observes that the probabilistic translation of naïve deductive confirmation leads to Bayesian probabilistic confirmation for any probability function, including the non-inductive or non-ampliative one known as Kemeny's logical measure function. In Chapter 4 of ICR I deal with genuine inductive probability functions, but neglect the non-ampliative type of confirmation. Indeed, naïve deductive confirmation leaves room for the claim that E d-confirms H just because E is a conjunct of H, i.e. $H = E\&H'$, where H' is logically independent of E. Although I would like to agree with Schurz that scientists are inclined to assign an ampliative character to confirmation whenever possible, I strongly disagree with the suggestion that non-inductive confirmation is no confirmation at all. In Section 7.1.2 of SiS, entitled "The landscape of confirmation," I introduce the distinction between structural and inductive confirmation, where structural confirmation is the probabilistic generalization of deductive confirmation, based on the logical measure function. That the latter type of confirmation, and hence the liberal account, should be taken seriously I like to demonstrate with my favorite example, that everybody would agree to say that the evidence of an even outcome (2, 4, 6) of a throw with a fair die confirms the hypothesis of a high outcome (4, 5, 6), whereas this has no ampliative aspect at all. However, typical inductive confirmation functions, like Carnapian or Hintikka systems, essentially combine structural and inductive confirmation, in such a way that the possibility of (purely) inductive confirmation in certain cases, e.g. $E=Fa$ and $H=Fb$, is paid by "counterinductive" confirmation going together with structural confirmation, e.g. deductive confirmation, e.g. $E=Fa\&Fb$ and $H=(x)Fx$. For further details, I refer the reader to SiS or to Kuipers (forthcoming).

Since I find none of the three arguments that Schurz advances against my liberal approach to HD confirmation convincing, I am not inclined to look for far-reaching sophistication, such as the relevant element approach. An extra reason is that this specific sophistication is, however elegant, technically rather complicated, whereas in practice scientists do not seem to use such advanced intuitions, let alone specifications of them.

Truthlikeness

Regarding truthlikeness, Schurz suggests that the number and importance of counterexamples to my basic definition and its refinements are so impressive

that we had better look for an alternative definition, viz. again in terms of relevant elements. Although the latter approach may have its advantages, I shall not really go into its separate and comparative evaluation, because most of the reported counterexamples and further counterarguments are either inappropriate or unjustified. But let me begin by saying that Schurz's presentation of my basic (or naïve) and refined account in Sections 7 and 8 are very transparent and largely correct. Just two points. First, Schurz writes in Section 7: "However, most theories of actual practice are not complete [even in the more specific sense of theories not implying atomic statements], and hence, Kuipers' concept of actual truthlikeness cannot be applied to them." (p. 239) However, in my first (1982) paper on truth approximation I extended my treatment of actual truthlikeness to incomplete theories, but since I came to believe that the actual truth is usually not approached by incomplete theories, but by complete or partial descriptions (at the end of ICR, Section 7.3.1 I hint at the easy extension to partial descriptions), I neglected the original extension. Hence, I fail to see it, as Schurz does, as a drawback of my definition to actual truthlikeness. My second point is that Schurz seems to be in doubt whether my definition of nomic truthlikeness might be intended in some factual sense, as opposed to a logical or conceptual sense. I do not understand how my Sections 7.2 and 8.1 could be understood in a factual sense, but I must concede that I state it only once explicitly and specifically for (my general version of) Popper's definition "When the definition applies, this means that certain areas [in Figure 8.1] are empty on logical or conceptual grounds" (ICR, p.180), but it must be clear from the context that the same applies to the empty areas in Figure 8.2 belonging to the basic definition.

Let us start by considering the purported counterexamples in Section 8. After noting that my definition of actual truthlikeness is not a limiting case of that of nomic truthlikeness and that if the latter is nevertheless used for dealing with the problem of actual truthlikeness, Schurz points out that Oddie's well-known child's play objection can be raised. Schurz's claim that, for similar reasons, basic nomic truthlikeness cannot adequately deal with theories that are incompatible with the nomic truth, notably theories using non-referring terms, seems more serious. Let us first turn to the general problem and then to non-referring terms. In general, we can indeed naïvely come closer to the nomic truth when we strengthen a theory that is incompatible with the nomic truth, that is, the nomic version of Oddie's child's play objection. However, in the naïve situation, in which models of theories are simply right or wrong, this is very plausible. Compare it with the situation of intending to grow cabbage plants and no weed, then (even) if you did not yet manage to grow some cabbages it is a step forward when you weed. Similarly, (even) if you do not yet capture correct models, you make progress by eliminating incorrect

models, assuming that you cannot improve them. However, the refined version can adequately deal with the nomic version of Oddie's objection (ICR, p. 254), as Schurz rightly remarks, but in the light of the foregoing I do not agree with his final statement "Still, it remains to be somewhat disappointing that a purely qualitative problem like the above one cannot be handled within the Kuipers' basic approach." (p. 241) Schurz is here referring to cases of incompatibility due to non-referring terms, to which I now turn.

Schurz seems to think that the incompatibility of a theory with the nomic truth may straightforwardly be due to the fact that terms may not refer. This, however, is not the case, and it is interesting to explain why. In my treatment of reference in Chapter 9, I make a sharp distinction between reference claims and other claims. To be honest, I consider my definition of reference to be one of the main innovations in ICR relative to my earlier publications. A term is referring iff it has effect on the set of nomic possibilities. In other words, it is a pure fiction when it does not narrow down the set of nomic possibilities. Moreover, a theory is supposed to make a reference claim for a certain term, if its addition to the remaining vocabulary implies a genuine restriction of the set of possibilities allowed by the theory, that is, if not all logical possible extensions of the relevant partial models of the theory become models of the theory. The result is that, for example, the phlogiston theory is compatible with the nomic truth as soon as its observational projection is compatible with it. Of course, we may assume that the phlogiston theory has general observational implications that are incompatible with the observational nomic truth, e.g. net weight decrease of burning material. Hence, the phlogiston theory is incompatible with the nomic truth, not because the term phlogiston does not refer as such, but because of its observational consequences.[2] As we have seen, it remains the case that we can come closer to the (observational and the theoretical) truth by simply strengthening the phlogiston theory, but this is not due to its non-referring nature, but to its false general test implications: in this case we do not eliminate models with non-referring terms, but models with wrong observational features.

I now turn to the second type of counterexample given by Schurz. He is again right in claiming that a new theory (e.g. that of Einstein) that is incompatible with an old one (e.g. that of Newton) can only be naïvely closer to the nomic truth when it entails the (relevant) nomic truth. However, in the "toy world" of naïve truthlikeness (ICR, p. 245) you need to add only correct models if you want to go further than rejecting (all) incorrect models. But the

[2] The crucial technical point is that reference claims essentially become meta-claims in my account. Hence, the claim "phlogiston exists" is not directly construed as a part of the phlogiston theory, but that theory, in contrast to the nomic truth, rules out certain states of affairs in such a way that it implies the reference claim with respect to 'phlogiston'. For details of this approach I have to refer to ICR Sections 9.1.2 and 9.2.1.

Newton-Einstein transition certainly is beyond the toy world. The transition is a typical case of idealization and concretization. More specifically, at the end of the refined approach in Chapter 10, 10.4.1, it is argued that the "Double Concretization theorem" holds, according to which a concretization of some idealized point of departure is a refined approximation of (itself and) any further concretization. Hence, if Einstein's theory is a concretization of Newton's theory and if the nomic truth is equivalent to – or a further concretization of – Einstein's theory, then Einstein's theory is closer to the nomic truth than Newton's theory.

For similar reasons, Schurz's third, artificial (*F, G, H*) example plausibly fails to satisfy the naïve condition, but it satisfies the refined condition, for a given domain of objects, all models satisfying *Y* are between models satisfying *X* and *N* and for each combination of (comparable) models in *X* and *N* there is an intermediate in *Y*.

In the criticism of stratified truthlikeness that follows, Schurz mainly contradicts my claim that a certain condition, "relative correctness," is weak. Unfortunately, he does not argue against my three technical general reasons (ICR, pp. 212-3), but his general type of example, viz. "if a scientific theory contains theoretically mistaken core assumptions, then all of its theoretical models will be infected by these mistakes," suggests that he may again have been thinking of non-referring terms. However, on p. 213 I announce and on p. 217 I explain why the condition of relative correctness is even trivial as far as non-referring terms are concerned. This has everything to do with my treating such terms as genuine fictions, that is, terms that do not play any role in shaping the set of nomic possibilities. To be honest, on pp. 212-3, I call a relatively correct theory a theory that is on the right track as far as the theoretical vocabulary (*Vt–Vo*) is concerned. This is a defensible way of speaking as far as referring (theoretical) terms are concerned, but it is of course highly misleading as far non-referring terms are concerned. The better paraphrase in this case would be a negative one, viz., restricting ourselves to a theory with only non-referring theoretical terms; such a theory is relatively correct in the sense that its theoretical terms as such, that is, neglecting the specific claim of the theory, do not exclude nomic observational possibilities.

In Section 8 Schurz finally introduces a quasi-quantitative counterexample to my refined definition of truthlikeness. The counterexample illustrates that the refined definition is, like the basic one, purely qualitative on the level of theories. Hence, further refinements may be desirable. However, the refined one already leaves room for quantitative specifications of the underlying notion of "structurelikeness." For this reason it opens the way to dealing with "truth approximation by concretization," e.g. the Newton-Einstein transition mentioned above.

In sum, apart from the last one, all purported counterexamples discussed by Schurz are either mistakenly construed or misinterpreted by calling them counterexamples. Hence, as in the case of HD confirmation, Schurz's arguments do not suggest looking for sophistication of truthlikeness in the direction of the relevant element approach. To be sure, he does not claim that the latter can handle the only counterexample that I find asking for further sophistication. Moreover, although I would now, unlike in the case of confirmation, not claim that my refinements are technically simple, they certainly appeal to procedures used in scientific practice, e.g. idealization and concretization, whereas I do not know of a similar appeal to relevant elements by scientists.

There remains one final remark. This is not the proper place to do full justice to the relevant element approach to confirmation and truthlikeness. Although I expressed my doubts about its need and its relation to scientific practice, Gerhard Schurz makes quite clear in the two sections on that approach that it can be developed in a technically elegant way.

REFERENCE

Kuipers, T.A.F. (forthcoming). Inductive Aspects of Confirmation, Information, and Content. Forthcoming in the "Schilpp volume" dedicated to Jaakko Hintikka.